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Solar Eclipses and Ancient History	*Samuel Jennings*	167
Errors of Transit Observations	*R. M. Stewart*	185
Microseisms	*Otto Klotz*	195
High Water in Lake Ontario	*Andrew Elvins*	209
Meetings of the Society at Toronto		211
" " " " " Peterborough	*E. F.*	211
Notes from the Dominion Observatory		214
Astronomical Notes		219
Summary Report of Weather in Canada for June and July		220

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THE NEW COMET.

JUST upon going to press we received a message from Harvard College Observatory announcing the discovery—photographically—on September 1st, of a comet with a long tail, by Prof. D. W. Morehouse at Yerkes Observatory, in R.A. 3h 20m, Dec. +66° 15′. At Kiel it was observed in a small telescope on September 3rd, R.A. 3h 19m 43s, Dec. +67° 14′ 42″; proper motion north-east.

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SOLAR ECLIPSES AND ANCIENT HISTORY

BY SAMUEL JENNINGS

THERE are few natural phenomena so impressive to the spectator as a total eclipse of the Sun. Even if he is looking for it, watching its development and already, to some extent, prepared for what he is about to see, the effect produced upon him by the few minutes of totality is most awe-inspiring.

But in people who do not know what is happening the most dread apprehension is aroused, and, as the climax approaches, they are filled with superstitious terror, the memory of which cannot fade away. It will be handed down from generation to generation, particularly when associated with some historical event which took place about the same time. Need we wonder, then, that events which have created so deep an impression have been recorded in ancient writings.

It might be supposed that such records would furnish for all time a ready and reliable means of determining with certainty the precise dates of such historical occurrences as have been thus signally marked.

Unfortunately, until within the last two or three years, such verification of identity of the ancient total solar eclipses has not been found possible, and at least one distinguished astronomer has come to the conclusion that all these eclipses must be put on one side as untrustworthy, or as too vague to be utilized.

Could these records of ancient eclipses be accurately identified they would be of inestimable value in enabling astronomers to correct and extend our knowledge of the apparent motions of the Sun and the Moon, at periods far apart from each other, the exact position of each of these bodies would be known, and data furnished upon which to base the necessary computations.

Many efforts have been made in the past to bring these various eclipses into harmony with each other, but without success. It has not, therefore, been felt that the information available was sufficient to warrant reliance upon these ancient dates in modern astronomical computations.

These difficulties have now, however, been overcome by the work of Mr. P. H. Cowell, F.R.S., Chief Assistant at the Royal Observatory, Greenwich. All these ancient eclipses now fall into their several places, and their respective zones of totality can be marked upon the map.

How Mr. Cowell reached these interesting and important determinations must now be explained. The results pass beyond the province of astronomical science, and penetrate into the domain of history and suggest reasonable solutions of many a difficulty which has hitherto perplexed students of the classics; they penetrate even into the realm of biblical literature.

More than two centuries ago, in 1693, Halley, afterwards second Astronomer Royal, showed that the month was very slowly changing in length. The amount of this change was first measured correctly by Prof. Simon Newcomb, the great American astronomer, who, in 1878, discussed the times of nineteen eclipses of the Moon recorded by Claudius Ptolemy, the astronomer of Alexandria, as having taken place between B.C. 721, and A.D. 136. But it never occurred to Professor Newcomb to take into consideration the possibility that there might be a change

in the length of the year, and when he turned his attention to the supposed records of ancient solar eclipses, they did not accord with his calculations. He therefore rejected them all as untrustworthy.

It must be admitted that there is considerable margin of doubt as to the interpretation to be put upon any one of these ancient references to eclipses when taken by itself apart from others. Several of the records in question were not written until long after the event ; the place where the eclipse occurred is not always definitely indicated, and, in one or two cases, the expressions used might accord with some merely atmospheric obscuration. But great reluctance must be felt in discarding the whole of such evidence.

Mr. Cowell's inquiry gives cause for concluding that the want of accord between these ancient records and Professor Newcomb's computations can be explained by one single hypothesis not in any way beyond the bounds of possibility. This hypothesis is that the ratio of the length of the day to the length of the year is very slowly changing, probably due to tidal friction ; though the Tables at present in use assume that there is no such change.

Mr. Cowell devoted the years 1903–1904 to a discussion of the modern observations of the Moon, those made at Greenwich in the last hundred and fifty years. He then took up the following five ancient eclipses of the Sun, viz.:—

Nineveh	B.C. 763,
Archilochus at Thasos	648,
Thucydides at Athens	431,
Agathocles near Syracuse	310,
Tertullian at Utica	A.D. 197,

and found them self-consistent. This consistency is the essence of the proof, for individually, as already pointed out, the records are often doubtful. Another point in favor of their acceptance is that the rate of change in the length of the month deduced from these eclipses agrees with Newcomb's result, derived in 1878 from the eclipses of the Moon.

The record of a sixth eclipse, the most ancient yet available to us, was then discovered by Dr. L. W. King on one of the cuneiform tablets in the British Museum. It would appear to refer to an eclipse of the Sun observed at Babylon, B.C. 1063. Here also, as with the five other records, there is some uncertainty in the interpretation, but this eclipse also exactly fits in with the other five.

The question therefore stands thus:—The astronomical tables at present in use do not account for these six eclipses as they are recorded. This may arise from one of two reasons, either—"an historian, poet, or chronicler recorded an eclipse in which the limit of totality was an unknown number of miles distant from the point of record, or the astronomical tables at present in use require some correction."* But if the descriptions of these six eclipses are thus inaccurate, then, seeing how widely separated they are in time and place, it seems beyond all probability that one single hypothesis as to the necessary alteration of the table would produce harmony between them all.

It is not sufficiently realized how rare an occurrence a total eclipse is at any particular spot. On the average such an event only occurs once in three hundred years for any given place. The last eclipse visible in England was that of 1724, the next to be seen in this country will be that of 1927, an interval of more than two hundred years, not for a single city, but for the country as a whole. The last visible in London was that of 1715, the next previous one visible in London was that of 873. It does not appear that another will be visible here for at least some six hundred years to come.

A little later on, Mr. Cowell examined three mediæval eclipses, namely, A.D. 1030, 1239, and 1241, besides two further ancient eclipses which he had previously left on one side. The records in these two instances had received identifications which were manifestly wrong, and they had been put aside by Professor Newcomb as either not being historical at all, or as referring to

* Prof. Simon Newcomb, *Monthly Notices of the Royal Astronomical Society*, Vol. LXVI., p. 34.

some phenomenon other than an eclipse. But both these, as well as the three mediæval eclipses fitted in completely with Mr. Cowell's computations.

The record of an eclipse of the Moon stands on quite a different footing from that of an eclipse of the Sun. There is no wide difference as to impressiveness between a total and a partial lunar eclipse. A total eclipse of the Sun is, on the other hand, a thing apart, and widely different in the sensations to which it gives rise from a partial eclipse, however little the latter may fall short of complete totality. Further, in a lunar eclipse the Moon is really darkened, hence the eclipse, whatever its magnitude, is the same over the whole of the hemisphere of the earth turned towards it at the time. A total eclipse of the Sun is total only over a very narrow belt of the earth's surface. From the nature of the case, therefore, eclipses of the Moon afford much less valuable evidence in lunar theory than eclipses of the Sun, and the whole series of nineteen eclipses given by Ptolemy, are not together worth more than a single solar eclipse. Mr. Cowell has, however, discussed these nineteen eclipses of the Moon, taking into consideration their recorded magnitude, a detail with which Professor Newcomb had not dealt, and found that they were in accord with his hypothesis of a very minute change in the relative lengths of the day and year.

The astronomical evidence therefore in support of this hypothesis rests upon seven solar eclipses before the Christian era, four solar eclipses since, and is consistent with the general testimony of nineteen lunar eclipses.

But the last two solar eclipses named are of especial interest from more than one point of view. They are those which are commonly known as "The eclipse of Larissa" and "The eclipse of Thales." Both these eclipses have been in dispute from the very earliest times, both have been wrongly identified, and the errors have been followed by the best historical authorities even down to the present day, with results which have considerably dislocated the history of that period.

Our knowledge of the first of these eclipses—that of Larissa—is derived from Xenophon, who, in his account of the Retreat of the Ten Thousand, writes:

"After this defeat the Persians retired and the Greeks, marching the rest of the day without disturbance, came to the River Tigris, where stood a large uninhabited city called Larissa, anciently inhabited by the Medes, the walls of which were twenty-five feet in breadth, one hundred feet in height and two parasangs in circuit, all built of brick except the plinth which was of stone and twenty feet high. This city, when besieged by the King of Persia, at the time the Persians were wresting the empire from the Medes, he could not make himself master of by any means, when it happened that the Sun, obscured by a cloud, disappeared, and the darkness continued till the inhabitants being seized with consternation, the town was taken." (*Anabasis*, Book III., chap. iv.)

Larissa has been identified as Calah, eighteen miles from Nineveh. The late astronomer Sir G. B. Airy, writing in 1856, identified the eclipse of Larissa with that of May 19th, 557 B.C. and showed that according to Hansen's Tables of the Moon, the narrow zone of totality passed nearly centrally over Larissa, and that there was no other eclipse within a period of forty years which could have been total at Larissa. But the growth of our knowledge in two directions has rendered Airy's identification impossible. The tables he used are inconsistent with modern observations of the Moon, and the zone of totality of this eclipse must have lain hundreds of miles to the south of Larissa.

Mr. Nevill, Director of the Natal Observatory, has further pointed out that the great progress which has been made in our knowledge of Assyrian history and chronology since the time when Airy wrote has enabled the actual dates of many events in Assyrian history to be determined with certainty, and that "Nineveh, Calah and other great cities of Assyria, disappeared from history before B.C. 600, and the very state of their ruins at the present time shows that the destruction was sudden and once for all. Every inhabitant perished or was transported in slavery to

some distant spot, and the cities were never allowed to be re-inhabited. The capture of Larissa to which the tradition refers must be the capture of the Assyrian Calah by the Medes and Babylonians prior to B.C. 600, and could not refer to the capture by the Persians of a town of whose existence there is no record, and of whose remains there is no sign amongst the existing ruins of Calah. As before remarked, the very state of the ruins shows that Calah was an Assyrian city when destroyed, prior to B.C. 600, and that its destruction was final."*

With the tables of the Moon, as corrected by Mr. Cowell, one eclipse, and only one, becomes total at Calah, namely, that of B.C. 603, May 18th. This date fits in remarkably with the chronology of the fall of Nineveh. The great siege began B.C. 609. Nineveh fell three years later. In 605, the Chaldeans defeated the Egyptians at Carchemish, to which city the latter would scarcely have ventured to advance if not only Nineveh, but also its dependent cities had already fallen. Then, as Mr. Cowell points out, the Chaldeans are lost sight of for three years, and in B.C. 601 or 600, they invaded Judea.† "The capture of Larissa exactly fits into this gap of three years in the current Assyriological knowledge."

It is, of course, conceivable that Xenophon's "great cloud" may not refer to a total eclipse of the Sun at all, but when we consider how deep an impression such an event as a total eclipse of the Sun must always have produced, especially upon the Assyrians, to whom the Sun represented their ruling deity, Asshur, there can be no real doubt about the identification. For, if the "great cloud" do not refer to the eclipse, then we are obliged to assume that the eclipse, which certainly did take place about the very time of the siege, passed without chronicle, whilst some unexplained darkening of the Sun, which, if merely meteorological, would have nothing of portentousness, must have attracted attention which the eclipse failed to secure.

*E. Nevill, *Monthly Notices of the Royal Astronomical Society*, Vol. LXVI., p. 409.

†P. H. Cowell, *The Observatory*, March, 1907, p. 138.

Referring again to the quotation from Xenophon, it will be seen that the historian had clearly fallen into error in placing the Eclipse of Larissa as occurring "when the Persians were wresting the empire from the Medes." The date usually assigned to this revolt was about B.C. 559. Astronomy tells us that a total solar eclipse did take place on May 19th, B.C. 557, and that it was visible as a very large partial eclipse in this region. It cannot, therefore, be doubted that Xenophon accepted the traditions of two separate eclipses, those of B.C. 603 and B.C. 557, under the misconception that they were one and the same. Similar vague traditions had misled Herodotus into serious error as to the date of the Eclipse of Thales, which took place on May 28th, B.C. 585. His record concerning this eclipse is as follows:

"War lasted between the Lydians and the Medes for five years: during this period the Medes often defeated the Lydians and often the Lydians defeated the Medes; and during this time they had a kind of nocturnal engagement. In the sixth year when they were carrying on the war with nearly equal success, on occasion of an engagement, it happened that in the heat of battle, day was suddenly turned into night. This change of the day, Thales, the Milesian, had foretold to the Ionians, fixing beforehand this year as the very period in which the change actually took place.

"The Lydians and Medes seeing night succeeding in the place of day, desisted from fighting, and both showed a great anxiety to make peace." (*Herod.*, Book I., chap. lxxiv.)

We then read that the matter in dispute was referred to arbitration: "Syennesis, the Cilician and Labynetus (Nebuchadnezzar) the Babylonian were the mediators of their reconciliation, these were they who hastened the treaty between them, and made a matrimonial connexion, for they persuaded Alyattes to give his daughter Aryennis in marriage to Astyages, son of Cyaxares."

Further on in the same Book I., chap. ciii., alluding to Cyaxares, King of Media, Herodotus writes: "It was he that fought with the Lydians when the day was turned into night as they were fighting; and who subjected the whole of Asia above

the River Halys. He assembled the forces of all his subjects and marched against Nineveh to avenge his father and destroy that city."

This passage would convey the impression that the fall of Nineveh took place *after* the war with Lydia, and it seems certain that Herodotus believed it was so, and he would therefore associate the eclipse of B.C. 610 with that said to have been foretold by Thales. Modern chronologists, following Herodotus, have adopted the same view. Woodward and Cates, Fisher, Baxter, the *Students' Bible* and others adopt the year B.C. 610, as that in which the Lydian war ended; whilst Hales, Clinton, Blair (last edition, 1904), give us the later eclipse—B.C. 603. Herodotus certainly fixed upon one or the other of these two eclipses as the Eclipse of Thales. If he adopted the earlier, that of 610, he was twenty-five years out of his reckoning, and if that of 603, he would be eighteen years wrong.

This mistake was not merely an error in chronology, it was more than that, because he built up his history upon that supposition, he found it necessary to account for twenty-five imaginary years between the accession of Astyages and the fall of Babylon in B.C. 536; therefore he represented Astyages as a much older man than he really was. The importance of the disclosure of this mistake will be seen later on.

The eclipse of Thales, as has been shown, was originally identified with that of September 28th, B.C. 610, but the path of the shadow in that year must have been too far north. Further, the war must have happened after the destruction of the Assyrian empire by the Medes and the Babylonians, and so have been later than B.C. 600. The eclipse of May 28th, 585 B.C., on the other hand, passed through Asia Minor, and the Sun set, according to Mr. Cowell's computation, totally eclipsed about east longitude 29.

This is exactly the phenomenon which appears to be indicated by the words of Herodotus in his reference to the "kind of nocturnal engagement" and that the "day was suddenly turned into night." We have no means of ascertaining the precise site of the battlefield.

It must be clearly borne in mind that there is no uncertainty as to the dates at which total eclipses of the Sun have occurred, nor even any doubt as to the general regions of the earth crossed by the shadow. The only uncertainty has been as to the exact position of the zones of totality.

There have been only four eclipses which could possibly have taken place within the period covering the historical events referred to in the quotations given above from Herodotus and Xenophon, and visible in the localities concerned, namely :—

September 30,	B.C. 610,
May 18,	B.C. 603,
May 28,	B.C. 585,
May 19,	B.C. 557.

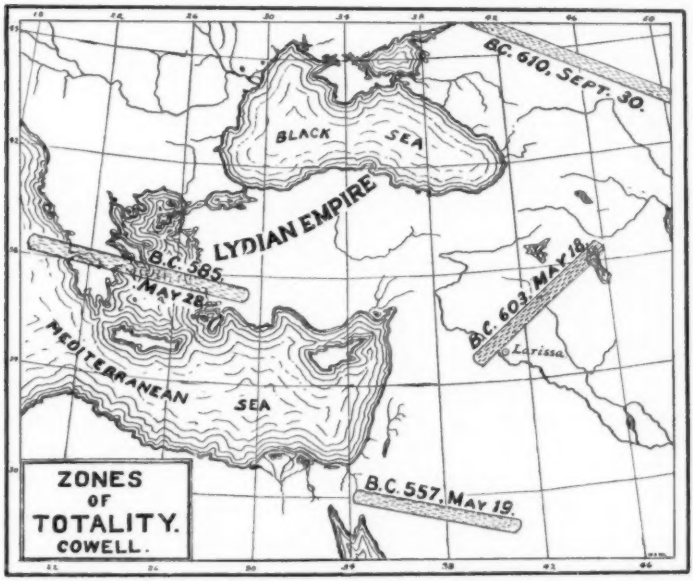
The tables of the Moon at present in use would give no one of the four as total at Larissa. It would be possible to suggest alterations in those tables which would make the eclipse of B.C. 557 total there, but if the same alteration were tried in the case of other eclipses, as, for instance, in the eclipse of Thucydides, at Athens, in the first year of the Peloponnesian war, it would be seen to utterly fail to satisfy the evidence, whilst we have now proof from Assyrian chronology that Larissa must have fallen before 600 B.C. Mr. Cowell's computations however at one and the same time, place the eclipse of 603 B.C. as total at Larissa, and satisfy the other ancient eclipses accessible to us. And the case is exactly analogous with the eclipse of Thales. It cannot have been the eclipse of 610, both from astronomical reasons, the path of the shadow lying too far to the north—and from historical, for it must have occurred later than the destruction of the Assyrian empire.

Both these eclipses, which, but a little while ago, were regarded as being outside any satisfactory identification, now fall into their proper places ; that of Larissa in 603, and that of Thales in 585.

A glance at the sketch map showing portions of the zones of totality of the four eclipses in question, as laid down by Mr.

Cowell, will sufficiently indicate the connexion between each eclipse and the locality and the historical event associated with it.

We gather from Herodotus that his information reached him through oral tradition, handed down through different channels. He selected that which appeared to him most likely to be correct. The occurrence of a total eclipse of the Sun would be sure to leave behind an indelible mark upon the memory. But, whilst



Paths of Ancient Eclipses.

the *fact* would remain clear, the period when it took place, as time passed, would tend to become more and more vague. It would appear to become more remote.

Herodotus was evidently unacquainted with the ancient observations of eclipses by the Chaldeans, who had discovered that solar eclipses recurred after intervals of eighteen solar years. This period they called by the name of "Saros." Had the his-

torian known this, it would not have been so wonderful to him that Thales, who had that knowledge, should have predicted an eclipse, and the eighteen years period would have at once indicated the eclipse which Thales foretold from his observations of the eclipse of 603, eighteen years after which came the eclipse of B.C. 585. Some few Greek and modern writers have in this way determined the correct date of the Eclipse of Thales, but, as we have seen, the weight of authority has followed the mistake of Herodotus.

So far, it will not be disputed that the determination of the eclipse of B.C. 585, marks with certainty the date of the accession and marriage of Astyages, and hence disposes once for all of the only serious difficulty in the way of the identification of Astyages with "Darius the Median" who, according to Daniel, received the kingdom of the Chaldeans "being about three score and two years old." (*Dan.*, v. 31).

This is no new suggestion. Niebuhr, Westcott and Vaux held this opinion, but admit a difficulty owing to the prevailing belief that Astyages must have been a much older man, even if he had been alive when Babylon fell in B.C. 536.

If, then, Astyages were sixty-two years of age in B.C. 536, he must have been born in B.C. 599–8, and would have been about fourteen years old at his accession and marriage, not an unusually early age in Eastern countries.

This forces another conclusion, viz., that Astyages could not have had a marriageable daughter (Mandane), said to have been the mother of Cyrus. Xenophon tells us that Cyrus was about fifteen or sixteen years of age when he hurled back the invasion of Media by Evil-Merodach in B.C. 559. (*Cyro.*, chap. iv., p. 16). Cyrus was therefore born about B.C. 576–5, when Astyages was twenty-three years old. So that it is clear that, whatever the relationship between them, Cyrus could not have been the "*grandson*" of Astyages.

We arrive, then, at three tolerably certain deductions from the astronomical locations of these four eclipses:—1st, that Astyages was not an elderly man at his accession and marriage;

2nd, that Cyrus was *not* his grandson; and, 3rd, that the question of his age is no longer the supposed insurmountable bar to his identification with Darius the Median.

It is well recognized that there are indications of a King Darius who had reigned in Persia before Darius Hystaspes, and it becomes more than probable that many references to "Darius," heretofore supposed to have been to the latter monarch, ought now to be understood as pertaining to the former—Astyages.

In *Daniel*, ix. 1. the father of Darius the Mede is stated to have been Ahasuerus. According to Scaliger and others, the names Cyaxares and Ahasuerus are identical, the one being a grecised form of the other, and no one disputes that Astyages was the son of Cyaxares I. the Medo-Persian king, who, in alliance with the Chaldeans, destroyed the Assyrian empire in B.C. 606, which event is alluded to in *Tobit*, xiv. 15. "But before he [Tobias] died, he heard of the destruction of Nineveh, which was taken by Nebuchadnessor and Assuerus." Moreover, Josephus says that Darius was known to the Greeks by another name. It is very unlikely that Herodotus ever heard the name Darius from Persians, who, according to Spiegel (*Eran.*, p. 88) "did not remember the names of their king, even the great Darius Hystaspes came back to them through the Greek traditions concerning Alexander."

Daniel may therefore be understood to tell us that Darius the Mede was the son of Cyaxares, in which case his identity is fully established.

One further point may be worthy of mention. In an inscription found by Sir Fenwick Williams of Kars, in 1850, upon the base of one of the pillars of the great temple at Susa (Shushan) is a statement of Artaxerxes (Memnon) tracing his descent in the usual grandiloquent language, through Xerxes to Darius Hystaspes. It concludes thus: "Darius, my ancestor, anciently built this temple, and afterwards it was repaired by Artaxerxes, my grandfather." The word "anciently" would appear to denote a Darius *before* the time of Hystaspes, probably Darius

the Mede. He had already mentioned Hystaspes, and had he meant his son, he would scarcely have used the word "anciently."

Xenophon introduces a king of Medo-Persia, whom he names Cyaxares II., son and successor to Astyages, of whom no trace can be found in any other writer. On the other hand, Herodotus distinctly states that Astyages was the last king of the Medes, and that he was succeeded by Cyrus. His statement receives direct confirmation in the apocryphal fragment entitled "Bel and the Dragon," which commences thus: "And King Astyages was gathered to his fathers, and Cyrus of Persia received his kingdom and Daniel conversed with the king and was honored before all his friends. And the Babylonians had an idol," etc. This agrees with *Daniel*, vi. 1–3, where Astyages is called *Darius*, and the period is clearly after the fall of Babylon, and may be accepted as evidence that he was reigning in Babylon (*circa* B.C. 535–4). We also have evidence that the same Darius was reigning in Ecbatana only a year or two previously. This is afforded in *1 Esdras* iii. and iv., where is a story concerning Darius and Zerubbabel, also narrated by Josephus (*Antiq.*, Book XI., chap. iii.) under the erroneous impression that the Darius here referred to was Darius Hystaspes, a chronological mistake arising from the original error of Herodotus in antedating the accession of Astyages by twenty-five years, as proved by the now indisputable date of the eclipse of Thales. Josephus, who was unaware of the identity of Astyages with Darius the Mede, offers a very lame explanation of a supposed visit of Zerubbabel to Darius Hystaspes fifteen years after the first return of the Jews to Jerusalem. He could not then have been the "young man" as he is described in *1 Esdras*. In the new light, however, this narrative finds its true historical place in B.C. 536, after the capture of Babylon by Cyrus, whose Decree in favor of the Jews had been sent to Darius (Astyages) for confirmation, but it was strongly opposed. (*Daniel*, x. 13). It is suggested that this narrative of "the three young men" relates the circumstances which led to the ratification of the Decree and the appointment of Zerubbabel to carry it out.

It will be remembered that Astyages and Cyrus were Joint-Kings, their reign over the Babylonian Empire began simultaneously, therefore "the first year of Cyrus" (*Ezra*, i.) was also "the first year of Darius" (the Mede) (*Daniel*, ix. 1) Astyages having died in the meantime, "the third year of Cyrus" (*Daniel*, x. 1) indicating that Cyrus was then sole king.

This may possibly explain why, when search was made for the decree of Cyrus by order of Darius Hystaspes fifteen years later, as recorded in *Ezra*, vi. 1–2, it was not found in the archives of Babylon but in the palace of Ecbatana in Media.

Still more surprising results follow the recognition of King Ahasuerus of the Book of Esther in this same Astyages of Medo-Persia. No more than the briefest outline of these results can be afforded on the present occasion.

Having the precise date of the accession of Astyages, it now becomes possible to determine what took place in the third, the seventh and the twelfth years of his reign, when certain events recorded in the Book of Esther occurred.

Reading these events into the narratives of Herodotus and Xenophon, which have been considered quite irreconcilable, a reason is at once shown why so many different and contradictory traditions were handed down to the historians. The political situation of the period comes to light, the mystery of the birth of Cyrus seems cleared up, the palace intrigues, the manœuvres of two hostile parties leading up to the revolt against Astyages, the cause of the bitter hatred of the Medes against Cyrus, the reason why Crœsus, King of Lydia, invaded Cappadocia, all these things become plain when read in the light of the record of the Book of Esther.

Starting with the accession of Astyages and the treaty of the River Halys under which he is married to the daughter of Alyattes, King of Lydia, in B.C. 585, his third year would be B.C. 582, when his Queen was disgraced and divorced. His seventh year would be B.C. 578–7, when he raised the Jewess Esther to be Queen in place of Vashti, and the plot to destroy the Jews

which took place in his twelfth year, would be in B.C. 573–2.

When it is remembered that the divorced Queen was the daughter of the then King of Lydia, and the sister of Crœsus, and that attempts were made to effect her restoration which nearly succeeded (*Esther*, ii. 1) but were strenuously opposed by the servants of the King, we see two political parties, the one, doubtless supported, and perhaps instigated, by the Lydian Court incensed by the outrage against the Queen which was a practical breach of the Treaty, the other, as firmly determined to avert what they deemed would have been a national humiliation. They were respectively, the Queen's party, and the King's party, and the speedy enthronment of a new Queen was intended to destroy the hopes of the Queen's supporters. Esther became Queen in B.C. 577. We have already seen that Cyrus was born in B.C. 576, the year following the marriage of Astyages to Esther, and Herodotus tells us that Cyrus was born in the palace of Astyages. There is no direct statement that Cyrus was the son of Astyages, but the inference, both from the place of his birth and the date thus indicated, suggestively point to the probability that his mother was Queen Esther. Mandane, said to have been the daughter of Astyages, could not have been the mother of Cyrus, but she might possibly have been, not his daughter, but his sister, married to Cambyses, the Persian, and the plot against the life of the infant, narrated by Herodotus, a purposely garbled version of what actually occurred, viz.:—that his life was in real danger from a palace intrigue, from which he was saved by Harpagus, who conveyed the child secretly to the care of Mandane in Persia, out of reach of his enemies, and Cyrus became their *adopted* son, this bringing Xenophon's story into line and accounting for the anxiety of Astyages, when Cyrus was brought home to Media when he was in his twelfth year, lest any harm should befall him. The King's determination that Cyrus should succeed him is shown by the promptness with which he was created Associate Military King, which was the immediate cause of the open revolt of the Medes against Cyrus, who was in command of the Persian army.

Ctesias, reflecting the bitterness of the defeated Medes, attributes their downfall to the treachery of Cyrus and Harpagus, he accuses the former of having revolted against Astyages and basely attacking him. Had this been so, how came it to pass that the fighting began in Persia and not in Media? The latter, Harpagus, had been appointed by Astyages to command the Median troops. Herodotus expresses his amazement that such an appointment was made: "as though the gods had deprived Astyages of reason." But, in the new light on the political situation, it becomes clear that, whilst the Queen's party supporting Vashti, with probable assurances of assistance from Lydia, had gathered strength in Media, Astyages was still determined to support Cyrus, and hence gave the command of the Median forces to Harpagus, whom he knew to be the friend of Cyrus, and sent him to lead them against the Persians, with the result recorded by history. We can perfectly understand why the Medes cried "treachery," the usual cry of a defeated party. There was evidently no hostility against the King, whose reign was, as we have seen, unbroken by this so-called revolt. Astyages continued to reign at home, whilst Cyrus pursued his career of military success abroad, first defeating the Lydian attack by Crœsus, and adding province after province and kingdom after kingdom to the Medo-Persian Empire, which, under the administrative genius of Astyages (Darius) became organized into one hundred and twenty-seven provinces. Astyages died in B.C. 535–4, and Cyrus then became sole king.

Here we have a new, and apparently at first sight, a fanciful history of the reign of Astyages, but on close examination, it will be found to afford a reasonable explanation to account for most of the difficulties admitted to attend every attempt to reconcile the conflicting records of Herodotus, Xenophon and others. Both these historians based their narratives upon century-old traditions, the one through channels hostile to Cyrus; the other regarding him as a veritable hero. We reject the idea that any of these ancient records are pure romance. They are founded upon fact, but distorted to suit the political bias of the narrators.

The uncertainty, which from the very first has attended the identification of the Eclipse of Thales, being now absolutely cleared up by the recent astronomical researches of Mr. Cowell, we are furnished with the certain date from which to reckon the events of the reign of Astyages, and are enabled to bring the successive incidents recorded in the Book of Esther into their proper historical places. These, in their turn, throw a new light upon the narratives of the ancient historians, the full importance of which it is scarcely possible to apprehend at present.

It is no small matter that the unconscious mistakes passed along over a period of two thousand years should now be rectified. Neither does it in the slightest degree reflect discredit upon the long succession of learned authorities who have dealt with this subject, because the information now at our disposal was not and could not have been theirs.

NOTE.—P. H. Cowell, M.A., F.R.S., remarks, in a paper appearing in *Monthly Notices R. A. S.*, Vol LXVIII., No. 2;—

"The fact that emerges from the discussion of the eclipses is that there is an unexpected rate of change in what may be termed the 'nodical year,' or period of revolution of the Sun relatively to the Moon's node.

"This abnormal rate of change may arise from secular changes in the motion of the Moon's node (other than that arising from the change of the eccentricity of the Earth's orbit round the Sun), or from secular changes in the motion of the Sun, or, of course, from a combination of both hypotheses.

* * *

"If the underlying cause be tidal friction, there is nothing improbable in the suggestion that slow changes are caused in the eccentricity and inclination of the Moon's orbit."

And again in *The Observatory*, January, 1908, p. 36: "A change in the ratio of the year to the day remained a necessary part of the the solution, although the change might be rather smaller than was first supposed."

See also *The Observatory*, December, 1907, p. 457.

The apparent change is approximately ten seconds of arc in a century.

(Ed.)

ERRORS OF TRANSIT OBSERVATIONS*

By R. M. Stewart

In a previous article,† discussing the relative accuracy of field transit observations with the observing key and with the travelling-wire micrometer, it was noted that for either instrument the actual average probable error of a time set was much greater than was to be expected from a consideration of the residuals of the separate stars of a set. It was pointed out that this must arise from some source of error systematic with respect to any one set, but varying from one set to another; some reasons were adduced, supported by actual results of observations, for supposing that this source of error consisted of a defective determination of azimuth. An investigation as to the best means of as far as possible remedying this defect has been carried out, the results of which are summarized below.

The average probable error of a time set was deduced from a comparison of sets observed on the same night by the same observer, using a reliable clock. In all about a hundred nights' work were examined, each consisting of at least two sets; the highest difference in observed clock error between two sets on the same night was 0·129 sec., the mean of all being 0·039 sec. As may be readily shown, this is equivalent to a probable error of about 0·023 sec.; on the other hand, the mean of all the probable errors obtained from star residuals was found to be only 0·011 sec.

As a further test of the validity of the hypothesis regarding azimuth error, the ten nights on which the discordances were greatest were selected for further examination. If the hypothesis were true, and assuming also that the portable instruments were fairly stable during the course of a single evening, we should expect that on those nights when the discordances were

*Communicated by permission of the Chief Astronomer.

† This Journal, July-August, 1907.

large they would be reduced by increasing the precision of the azimuth determination. This was done by grouping all the observations on any night and reducing for a single value of azimuth, which should evidently be better (if the instrument were stable) than either of the values previously applied in the reduction of the sets separately. Finally, a value of the clock correction was found from the time-stars of each set separately, by applying this value of azimuth to all. The effect of this method of reduction was materially to reduce the discordance in every case, the reduction varying from 40 per cent. to 86 per cent., the mean of all being reduced to one-third of its former value (0·102 sec. to 0·034 sec.)

Accepting the conclusion that large errors are usually due to defective azimuth determinations, two courses are open as a remedy. We must either recast the distribution of time-stars so that an erroneous azimuth will have a negligible effect, or we must increase the weight of the azimuth; the latter may be effected either by increasing the number of polar stars in a set, or by increasing the accuracy of the separate observations.

Some years ago the Prussian Geodetic Institute adopted a change in their method of observing, designed to eliminate the same effect. The time sets used by them now consist of stars culminating within a few degrees of the zenith, both north and south, so that azimuth error is almost wholly eliminated; in addition, one or two polars of high declination are observed for an approximate value of the azimuth, which is in this case sufficient.* The observation of zenith stars is, however, feasible only with a transit of the "broken" type, where the eye-end is situated at the end of the axis; as the transits in use in this country are for the most part of the ordinary straight-tube type, precluding the comfortable, and therefore accurate, observation of stars within five or ten degrees of the zenith, this method of observing could not be adopted. The same general effect could be obtained in low latitudes by grouping the time-stars north and south of the zenith, and as close to it as possible, but for

* *A. N.*, No. 3699, Band 155.

latitudes north of 30° or 35° the number of standard north stars available within reasonable limits of right ascension is rather limited. To the influence of this fact may be traced the lines of evolution of the time set as used in Canadian work up to the present. The normal set consisted of twelve or fourteen stars, one half of this number being observed "clamp west," the other half "clamp east." In each clamp was one polar of 75° or 80° declination, the remainder being distributed from the equator upwards; the general principle was adopted of making $[A]$, that is $[\sin \overline{\varphi - \delta} \sec \delta]$, as nearly equal to zero as convenient for each set; this condition would be the correct one to eliminate azimuth error *if the accuracy of observations on north and south stars were equal*; otherwise the effect of the azimuth error predominates.

In seeking a solution of the difficulty, the question of increasing the accuracy of observations on polar stars was first taken up. During the summer of 1907 a travelling-wire micrometer was being constructed in the Observatory workshop, and the opportunity was seized of introducing such modifications as to make possible a change in the method of observing. It was designed that on slow-moving north stars four separate groups of observations should be taken, distributed symmetrically on either side of the line of collimation; the space between the groups was sufficient to allow of reversal of the instrument if desired. On the completion of the instrument in October, special observations were made on a number of polars, both above and below the pole, on several nights. Four groups of observations were made on each of the 74 stars observed, the instrument being reversed in every case between the two middle groups. A comparison of the mean of all four groups (for any star) with the means of each symmetrical pair, gave the increase in accuracy obtained by increasing the number of groups from two to four; a comparison of the mean of each symmetrical pair with the mean of each group composing it (after reduction to the meridian of the instrument) gave the advantage arising from reversing the instrument during the observation. By making the test in this way the comparisons were practically freed from

the influence of all variable conditions, such as "seeing," variation of instrumental corrections, errors in star places, etc., while the amount of labor required was much less than if an attempt had been made to determine the probable error of observations of each type by independent observations. The mean increase of efficiency due to reversal was found to be 67 per cent., that due to further doubling the number of groups only 16 per cent. additional. It was concluded that the principle of reversal was well worth adopting, but that in general the advantage gained by taking more than one group of observations in each position of the instrument would be practically eliminated by the increase of time required for the observation and by the additional labor of scaling the chronographic record; it could be resorted to, however, in regions of the heavens particularly destitute of polars, or on nights when the weather was broken. It was decided also to extend the principle to the observation of south stars as well, it being thus possible to obtain the same accuracy with a smaller number of stars.*

The difficulty due to scarcity of north stars being thus partially overcome, it remains to consider the most efficient ratio of distribution of the north and south stars, as well as the most suitable zenith distances of each. It may be shown theoretically that maximum efficiency involves two independent conditions:—(1) the "collimation" condition that the stars in the two clamps should be identical as regards declination; this is exactly fulfilled by reversing on each star; (2) the "zenith distance" condition that all the stars should culminate exactly in the zenith; this, as pointed out above, can not be even approximately fulfilled; consequently it becomes necessary to inquire how much deviations from this condition will affect the accuracy of the result. This is a problem which does not by any means yield readily to general treatment, but by imagining the set broken up into two groups, one north and the other south of the zenith, the stars of each group being of the same declination, the problem is somewhat simplified. This will not involve any

*This principle is also in use by the Prussian Geodetic Institute.

great departure from the actual conditions, since, though the groups must usually be spread over a considerable interval in declination, the distance between the groups is in general considerably greater.

From each observation we obtain an observation equation of the form

$$p A a + p \Delta T = pl$$

where a and ΔT are the azimuth and clock errors, $A = \sin \overline{\varphi - \delta}$ sec δ, l the observed clock error uncorrected for azimuth, and p the weight of the observation. If an observation on an equatorial star be taken as the standard, we may assume

$$p = \frac{1}{1 + \alpha_2 \tan^2 \delta},$$

where α_2 depends on instrumental conditions, such as speed of driving heads, magnification, etc. Its value for the transits used at Ottawa was found experimentally to be about one-half. Combining the observation equations by least squares and solving, the weight of ΔT is given by

$$W = [p] - \frac{[pA]^2}{[pA^2]}.$$

If we have n south stars at declination δ and n' north stars at declination δ' this reduces to

$$W = \frac{n\,n'\,p\,p'\,(A - A')^2}{n\,p\,A^2 + n'\,p'\,A'^2} \quad . \quad . \quad . \quad . \quad (1).$$

Supposing the whole number of stars in the set, $n + n' = N$, to be fixed, and supposing δ and δ' also fixed, we may find the best distribution, *i.e.*, the best values of n and n', by differentiating (1) with respect to n and n' and introducing the condition $dn + dn' = 0$. This gives the condition

$$n^2\,p\,A^2 = n'^2\,p'\,A'^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2).$$

Introducing this condition in (1) and eliminating n and n' we get

$$W = \frac{p\,p'\,(A - A')^2}{\left(\sqrt{p\,A^2} + \sqrt{p'\,A'^2}\right)^2} N \quad . \quad . \quad . \quad . \quad (3),$$

the surd quantities being taken with the positive sign. This

value of W is the maximum that can be obtained from N stars grouped at declinations δ and δ'.

Differentiating (3) with respect to δ, and substituting the values of p, p', A and A' in terms of δ and δ', it may be shown that $\frac{dW}{d\delta}$ is positive; in like manner, differentiating with respect to δ', $\frac{dW}{d\delta'}$ is negative. Hence W is increased either by increasing δ or decreasing δ', that is, by diminishing the zenith distance of either north or south stars, or both, *provided the ratio of n and n' be at the same time so varied as to continue to satisfy* (2). This conclusion includes the case of north stars at lower culmination.

The best set, then, can be obtained by the use of stars, both north and south, as near to the zenith as they can comfortably be observed. We have yet to consider, however, whether there are any practical considerations, such as the number of stars available at different declinations, which will interfere with the adoption of this principle. To get a clearer idea of the *amount* of variation in precision corresponding to variations in declination, we may compute W, and also n or n', for special values of δ and δ'.

TABLE I.—MAXIMUM WEIGHTS OF SETS OF 12 STARS FOR LATITUDE 45° 24′.

	$\delta = 0°$		$\delta = 10°$		$\delta = 20°$		$\delta = 30°$	
	W	n'	W	n'	W	n'	W	n'
$\delta' = 60°$	10·1	8·3	10·5	7·8	10·9	7·0	11·3	5·7
$\delta' = 70$	8·8	6·7	9·4	6·1	10·0	5·3	10·7	4·1
$\delta' = 80$	7·5	5·7	8·2	5·1	9·1	4·3	10·1	3·2
$\delta' = 90$	6·2	5·0	7·1	4·4	8·2	3·7	9·4	2·7

In Table I. these quantities are shown, as computed from (2) and (3), for sets of twelve stars observed at the latitude of Ottawa (45° 24′). The unit of weight has in this case been taken as an observation on a single zenith star; the values for 90° have been added simply to show the tendency as δ' is increased beyond 80°. The desideratum is so to choose δ and δ'

that the value of W may be as high as possible without unduly increasing n'. By increasing δ, n' is markedly decreased, while W is somewhat increased ; for both reasons, but more especially the former, the south stars should be observed as near the zenith as practicable ; in actual practice, in order to get a sufficient number of stars within a reasonable time, this will involve a mean zenith distance of not less than 20°. In the case of north stars a decrease in δ', while it increases W, also increases n' in a much greater ratio ; on the other hand, the number of standard stars available is about the same between 70° and 80° as between 60° and 70°. Hence, instead of choosing 65° (a zenith distance of 20°) as the mean declination of north stars, it will be better to choose 75°, as this will not involve a great decrease in W, while it will permit us to observe nearly the proper ratio of polars in the set. For $\delta = 25°$ and $\delta' = 75°$ we get $n' = 4{\cdot}2$; that is, the best results for these declinations are got by observing in the ratio of about one north star to two south stars, instead of one to five or six as heretofore. We might still further lower n' without very materially decreasing W by increasing δ' beyond 75°, but, in addition to the fact that the difficulties of observing with the micrometer are greatly increased for stars much beyond 80° declination, the number of stars available decreases more rapidly than n' ; consequently the best declination for polars (near the latitude of Ottawa) is probably from 70° to 82° or 83°.

Tables II. and III. show the weights, computed by formula (1), of sets of twelve stars for the same declinations as above, the numbers of polars being respectively two and four. An inspection of the corresponding weights in both tables shows that the advantage gained by increasing the number of polars at the expense of south stars is very considerable.

It may be remarked here that the figures in Tables I., II. and III. are substantially the same (if for equal declinations we substitute equal zenith distances) for latitudes 35° or 55° as for 45°, both W and n' being in all cases slightly increased for 35° and slightly diminished for 55° ; hence the same general conclusions hold over a considerable range of latitude, being altered

merely by the changes in the relative number of stars obtainable north and south of the zenith.

TABLE II.—WEIGHTS OF SETS OF 10 SOUTH AND 2 NORTH STARS FOR LATITUDE 45° 24′.

	$\delta = 0°$	$\delta = 10°$	$\delta = 20°$	$\delta = 30°$
$\delta' = 60°$	3·4	3·9	4·9	6·8
$\delta' = 70$	4·1	5·1	6·5	8·8
$\delta' = 80$	4·4	5·6	7·2	9·5
$\delta' = 90$	4·2	5·5	7·1	9·2

TABLE III.—WEIGHTS OF SETS OF 8 SOUTH AND 4 NORTH STARS FOR LATITUDE 45° 24′.

	$\delta = 0°$	$\delta = 10°$	$\delta = 20°$	$\delta = 30°$
$\delta' = 60°$	6·4	7·3	8·5	10·4
$\delta' = 70$	7·1	8·2	9·5	10·7
$\delta' = 80$	6·9	8·0	9·1	9·9
$\delta' = 90$	6·0	7·1	8.1	8·9

TABLE IV.—COMPARATIVE WEIGHTS OF TIME SETS.

No. of Polars	δ'	No. of South Stars	δ	Method of Observation	Weight
* 2	76° 45′	10	14°	Without reversal	6·0
† 2	78° 19′	10	14°	" "	6·1
‡ 2	83° 30′	10	14°	" "	6·2
2	75°	10	25°	" "	8·0
2	"	12	"	" "	8·6
2	"	4	"	By reversal	8·3
2	"	6	"	" "	10·5
3	"	6	"	" "	12·5
3	"	8	"	" "	14·8
4	"	8	"	" "	16·6

In Table IV. are collected the weights of a few typical sets, showing the advantages to be gained by decreasing the zenith distance of the south stars, by increasing the relative number of polars, and by reversing during each observation. In the case of the sets observed by reversal the weights obtained by formula

* Values of δ and δ' actual means from a large number of sets observed in the past.

† Condition $[A]=0$ fulfilled.

‡ If $\delta = 14°$, $n = 10$, $n' = 2$, W is a maximum when $\delta' = 83° 30'$.

(1) have been increased by two-thirds, in accordance with the results arrived at above. In all cases the weights given by the formula have been multiplied by $(1 + \frac{1}{2} \tan^2 \varphi)$, so that the unit is an observation (without reversal) on a zenith star.

It is, of course, to be remembered that in all that precedes, the only errors taken account of have been those inherent in the actual observation; it has been tacitly assumed, for instance, that the adopted correction for level, as applied to any one set taken as a whole, does not differ appreciably from its true value, that the collimation of the instrument does not change with variations in position, that anomalous changes in refraction do not affect the result, that the residual pivot errors are negligible, etc. Though none of these assumptions is strictly true, the actual evidence shows that these errors are overbalanced by those arising from defective azimuth determinations; consequently the introduction of the consideration of relatively insignificant errors would tend to alter merely the relative and absolute *values* of the theoretical weights, but not their *sequence*. We must not expect, then, to gain the whole advantage shown by the differences in the weights in Table IV., but of some advantage, and that a quite considerable one, we may be assured.

The method of determining the azimuth by observing stars both above and below the pole has not been considered, because in some preliminary observations indications of systematic differences were found; pending a fuller investigation of this question it was decided not to use such sets for the present.

There is still another matter to be considered. We have considered as mutually exclusive the two alternatives of observing polars between, for example, declinations 60° and 70°, or between 70° and 80°. We might, however, distribute them between 60° and 80°; this would involve (for the best results) an additional increase in the number of polars observed, but this would be more than compensated for (so far as scarcity of polars is concerned) by the additional number available. In making the computations it has always been the custom here to give all observations equal weight, and so far as the value of ΔT is concerned

this is perfectly legitimate, *provided the polars are not spread over a wide range of declination*; otherwise we cannot get a correct result without weighting the observations for different declinations. Whether the advantage to be gained by opening a wider range of declination for north stars is more than sufficient to compensate for the increased labor of computation, is a matter to be decided by experience.

So far as concerns actual results of observation the data for a comparison are as yet rather meagre, as the results of the present summer's regular work have not yet been reduced. Enough knowledge has been gained, however, to show that the method of observation is perfectly feasible, and so far as the possibility of a comparison goes, the improvement in accuracy seems decided. Contrary to what might be expected, there is very little additional labor involved in the observation of a set. The scaling is considerably increased, but the computation is very much simplified; even if a system of weighting should have to be adopted the computation would still be on the whole rather simpler. It may be added that the observers here are all in accord with the writer as to the necessity for an improvement in the methods of observation, and as to the efficiency of the remedy adopted.

DOMINION ASTRONOMICAL OBSERVATORY,
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MICROSEISMS

By Otto Klotz

BY this designation are included all pulsations not directly attributable to what are generally known as earthquakes, that is, of abrupt, more or less violent, and momentary movements of the earth's crust, the effect of which may, however, continue for some hours. Attempts have been made to classify these microseisms according to their cause, but so far without complete success. During the past year the writer has paid considerable attention to these disturbances, and in doing so has studied and compared the daily seismograms with our weekly aneroid barograms and also with the daily weather maps, which give the isobars at 8 A.M. for Canada and the United States, roughly between latitudes 25° and 55°, and the Atlantic and Pacific oceans. The average time of the beginning of the seismogram sheet is about 10 A.M., so that the above isobars and gradients dependent upon them are for a time preceding the former by two hours. From the examination of the local barogram alone not much information can be gathered as to the behavior of the seismograph except when very rapid and marked fluctuations, say a millimetre or more, take place in the pressure, comparable with the "pumping" of a mercurial barometer at sea. The barometer may show little or no change in pressure at a given place yet areas of "High" and "Low" (barometer) may be rushing along to the north and south of it, setting up vibrations or pulsations of the earth's surface that may be markedly felt at a given place by the seismograph. Similarly from a large rise or fall of the barometer during 24 hours at a given place alone, we can draw no gradients to determine the atmospheric movements; the position of the isobars and "Highs" and "Lows" being unknown. We have simply the record of the vertical movement of pressure at one point. A table has been constructed, giving the microseisms, although not when showing merely the faintest trace

here and there, recorded between July 1st, 1907, and March 1st, 1908; also the state of the barometer for the respective day taken from the weekly barogram sheet of an aneroid; and lastly the position of the areas of "High" and "Low" barometer at 8 A.M. of the respective day, taken from the daily weather maps covering Canada and the United States. The isobars are drawn at intervals of a tenth of an inch. The normal to the isobars is called the gradient, and when spoken of generally refers to the gradient between a "Low" and "High" passing through Ottawa. The maximum absolute amplitude of the microseism is expressed in microns. It is to be noted that the beginning of each seismogram is about 10 A.M., that is, two hours after the time of the isobars of the weather maps.

The object of the tabulation is to show various phenomena of the same time, and trace if possible any connection or relationship between them. The word "Gulf" refers to the Gulf of St. Lawrence.

In connection with the relationship that may exist between microseisms on the one hand and the statical and dynamical conditions of the atmosphere on the other, another table has been compiled. In it are given all the strong winds and gales predicted at 8 A.M. for the Ottawa and Upper St. Lawrence valleys for the respective day, including the period July 1st, 1907, to March 31st, 1908, so that at a glance we can see whether more or less strong microseisms were accompanied by strong winds or gales, and on the other hand whether strong or high winds produced marked microseisms. As the Observatory is not yet supplied with an anemometer and pressure gauge for comparison of the dynamical conditions, we are at present dependent upon the daily forecast.

It may be stated at the outset, before discussing the preceding data, that there is never a day in the year on which some trace of microseisms can not be seen on a seismogram from a Bosch photographic seismograph. It is all a matter of degree. That microseisms should be ever present is but natural, for the earth is in a continual state of stress and strain, many varied

and different causes contributing thereto. The term microseisms as here used excludes any deviations of the vertical or movements of the zero position of the pendulum. Some writers have divided microseisms into "earth tremors" or "pulsations," and "earth pulsations" or "pulsatory oscillations." The writer, however, from the seismograms at this station sees no reason for this division, as it is not at all evident from them that the contributory causes, whatever they may be, manifest themselves in such a manner as clearly to differentiate themselves. Furthermore, from the examination of the seismograms the oscillations of the pendulum are excluded; on the one hand, from the frequent change of period on the same seismogram, which would be inadmissible for a pendulum, and on the other hand, if the pendulum were made to oscillate we should expect to see the damping effect in the decrease of amplitude, and a more or less sudden beginning, unless the oscillations of the earth particles themselves were of a period commensurable with that of the pendulum, which, of course, is sometimes the case. It is evident that a photographic registering apparatus with high magnification will record microseisms when a seismograph with mechanical registration will draw only a straight line.

Of the contributing causes to stresses and strains and manifesting themselves as microseisms, we may consider:—secular cooling of the earth; unequal heating and the radiation during the day and night; statical effect of atmospheric pressure, areal or local; dynamical effect of atmospheric pressure, areal or local; precipitation, as rain or snow.

The vanishingly small effect of secular cooling, whatever its constants may be, becomes evident from the fact, that, although it is ever present, and its manifestations would be of a constant nature, the recorded microseisms are of the most fluctuating character both in time and magnitude, completely masking the effect of secular cooling. The daily alternations of unequal heating and radiation during the 24 hours are not shown by their effect on microseisms. The case of precipitation is similar in regard to microseisms to the preceding. It may be noted that

the stresses set up over large areas, hundreds of miles in extent, by differential loading of rain, is small compared with that of barometric pressure. Taking an area, say of a thousand miles with a rain-fall of an inch, which is a pretty heavy rain, and decreasingly distributed, we would have a maximum pressure of a little over one-thirtieth of a pound per square inch, and the rain-pressure diminishing to zero for the edge of the area. An average barometric gradient, on the other hand, over such an area would be several times as great, due to a differential atmospheric pressure equivalent to about three-tenths of an inch of the mercurial barometer. The rain-pressure may make itself, however, felt locally as has been observed. The result of a heavy rain-fall soon fills the valleys and streams much beyond the direct precipitation on them, so that this loading and bending of the surface may become a measurable quantity by an observing station in the neighborhood. This effect is, however, one of tilting, of change of vertical or change of pendulum zero and not of microseisms, the subject at the moment under discussion.

The effect of difference of atmospheric pressure and of change of atmospheric pressure may be manifested in two ways by the seismograph. We are here dealing with large areas, say 1000 miles in extent, for local barometric conditions have little or nothing in common with microseisms. In the one case, considering the earth as having an elastic crust, the pier is tilted towards the area of greatest pressure, in consequence of which the pendulum will move in that direction, *i.e.*, its zero line will be displaced. Besides this effect of statical loading, there appears to be no doubt, based on the records here, that vibrations are set up by this statical loading, quite apart from the dynamical effect of change of pressure. In the other case, by change of pressure over a wide area vibrations are set up on the earth's surface, and these may be produced by two causes from the one phenomenon. The one of these is the passage of "Highs" and "Lows" over the surface, equivalent to the dragging of a weighted meniscus over the surface; and the other is the winds set up or resulting from the atmospheric gradient due to difference of pressure.

The action of the winds would be most likely from frictional resistance along the surface of the earth rather than from impact on unevenness of surface or obstructions. In studying various phenomena collectively in an investigation for correlationship, considerable restraint must be exercised not to draw conclusions as to cause and effect from a limited number of coincidences. For a conclusion once drawn is apt to become an obsession to the investigator, and he is more or less blinded to facts that do not fit his theory.

In examining the record of microseisms the first question that presents itself is whether the recorded motion is that of the ground or of the pendulum, in the first case the pendulum acts as a steady mass or point, while in the latter case it is set oscillating either by impulses from the ground or by an undulatory movement of the ground. Let us consider the case of microseismic record of the "Sawtooth" type, where we see regular and almost wholly uniform oscillations kept up for hours and longer. If in this case the pendulum actually oscillates it will do so with the period inherent to it. After receiving the first impulse or impact let us suppose it to oscillate, if no further impact were received the oscillations would soon die out and the amplitudes would decrease in the given ratio of the damping coefficient. When a second impulse is given the pendulum will continue its uniform swings provided the time interval from the preceding impulse is that of the period of the pendulum or a multiple thereof. Is this not the case, then we will have interference and this would be shown on the record. But such interference is not present in our supposed diagram, hence we must conclude that even if we admit that the diagram is a record of the oscillations of the pendulum, we see that in reality it is only a counterpart of the actual movements of the ground, that is, of horizontal to-and-fro motions of the earth particles. If the pendulum is kept swinging uniformly it can only be done so by some force acting at intervals of the period of the pendulum. As indicated, this may be done by the periodic oscillating movements of the earth particles; or the same effect may be produced by rhythmic undu-

latory movements of the ground. Now, the period of microseisms recorded here lies mostly between 5 and 6 seconds, which it may be remarked is also approximately the period of the two pendulums, and the period of the undulatory movements manifested in the "principal portion" of tectonic earthquakes is 20 seconds or more, so that for microseisms we find the period only about one quarter of the preceding, provided we admit that the record of the microseism is due to undulatory motion and not to horizontal movements. The shorter period might perhaps be assigned to a far thinner part of the crust of the earth being affected in the microseisms than is involved in the undulatory motion connected with microseisms. As an analogy we may give the short period of the ripples in water from a breeze, or the much longer one of waves from a storm when a greater depth of water is involved in the motion.

A priori reasoning does not appear to furnish a conclusive reply to the question whether the microseisms are attributable to the horizontal or to the undulatory movements. However, the consideration of the simultaneous occurrence of microseisms together with certain atmospheric or barometric conditions leads to the conclusion that microseisms are mostly attributable to horizontal displacements.

Having made daily comparisons with the seismograms, local barograms and weather maps, the following conclusions have been deduced. It is believed that identical atmospheric conditions prevailing over different parts of the earth's surface will not necessarily produce similar microseisms, as these are affected by the elasticity of the particular area under consideration, also by the geological formation, the presence of well-marked dykes, faults, and by the proximity of large sheets of water, the ocean. One effect of the proximity of the ocean caused by barometric pressure is the change of the level of the water, quite apart from the tides, and this change through loading or unloading along the coast produces a displacement of the pendulum zero, referred to in another place. In the sea, then, we have the dual effect of the direct barometric pressure and the correlated

one of displacement of the water, while on land we have only the former.

The feature to strike one most in the above comparisons is that when marked microseisms are present we are almost certain to find in the morning of the day of record for the following 24 hours an area of "Low" about the Gulf of St. Lawrence. That is, the condition of "Low" in the Gulf precedes the record of marked microseisms. The greater part of the Gulf is less than 150 fathoms deep. Through it runs a "deep" from the mouth of the St. Lawrence (Matane), along the south of Anticosti, passing between Cape Breton and Newfoundland reaching a depth of 250 fathoms before joining the Atlantic ocean. This deep is over the eastern part of the Great St. Lawrence and Champlain Fault, shown on the Geological maps, for nearly 700 miles. The waters about Nova Scotia and Newfoundland are all within the 150 fathom line, so that the "Lows" over the Gulf and Sable Island are over waters the greater part of which are less than 150 fathoms deep. The distance from Ottawa to the Gulf is about 700 miles, direction east-north-east; and from Ottawa to the nearest broad waters of the Atlantic, off the State of Maine, 300 miles, direction east-south-east.

Next to the presence of a "Low" in the Gulf coincident with microseisms we find the isobars to cut the valley of the St. Lawrence (in which lies the great fault) at right angles, that is, the gradient is along the St. Lawrence valley, which is in general parallel to the Atlantic coast, and to the line of the Alleghany mountains.

Furthermore, it is found that if a "High" prevails along the South Atlantic coast, northward from Florida the microseisms are intensified.

The passing of "Highs" or "Lows" across the coast-line, *i.e.*, from land to water, is not found to be marked by the occurrence of microseisms. As the whole atmospheric movement is for Canada and the United States from west to east, it is uncommon for a "High" or "Low" to cross the coast-line *from* the Atlantic *to* the continent.

It appears that the reversal of the position of "Low" and "High" with reference to the Gulf for the former is not so closely associated with the subsequent appearance of microseisms as obtains in the case first stated.

When there is a persistence of "Low" in the Gulf and "High" on the Atlantic coast to the south as indicated, the microseisms set up in the first instance become intensified in amplitude, so that the maximum microseisms are not necessarily coincident with the greatest difference of pressure. It appears that the difference of barometric pressure is in the first instance responsible for the microseisms, and when favorable conditions continue the microseisms will increase in amplitude, although the pressure difference may have decreased. Furthermore, another condition is that the line of "High"–"Low" preserves its direction along the St. Lawrence valley.

When a "Low" with even very steep gradients is to the west, say over the Lakes, and "High" on the lower St. Lawrence or Gulf, microseisms are generally weak or even absent altogether, although there are exceptions. This is not the case when the "Low" is to the east, especially when over the Gulf. When the "Low" with steep gradients moves up to Lake Erie by 8 A.M. of the day of the seismogram we may expect to see the beginning of marked microseisms, which increase as the "Low" moves down the St. Lawrence towards the Gulf. From the immediately preceding it is seen that the microseisms give no indication of the approach of a "Low" or storm centre, but on the contrary are the result of the passage of a "Low" and especially of its presence in the Gulf. Some investigators believe and are in hope the microseisms may be the forerunner of coming weather conditions, and hence may assist in making forecasts. The seismograms examined here are not very encouraging on that point, the microseisms indicating rather "that we have had weather, than that we are going to have weather." This prognostication refers to the microseisms and not to the effect of bending, or displacement of the pendulum zero, brought about by unequal pressure over a large area. The writer is not as yet

prepared to say whether the approach of a "Low," with the consequent lifting or rising of the earth's surface, is a distinctly measurable quantity as registered by our seismograph, for the measurements of the two components of the change of pendulum zero for the year have not yet been tabulated and critically compared with the movements of "Highs" and "Lows" in the eastern part of the continent.

In connection with the bending of the earth's crust due to difference of atmospheric pressure, reference may be made to two exceptionally marked results obtained by Professor Omori. The conditions prevailing were undoubtedly unusual, and the angular values obtained so large, that one would be led to conclude that under ordinary atmospheric conditions, *i.e.*, of barometric difference, the tiltometer would always respond to a degree that was readily measurable. Of this fact, however, the text in the references given below say nothing.

In No. 21 of the *Publications of the Earthquake Investigation Committee in Foreign Languages* is given the record, with its interpretation, of a horizontal pendulum at Tokyo during a storm, October 10–11, 1904. The weather chart which accompanies the paper shows that a "Low" (750 mm.) prevailed and moved along the east coast of Japan past Tokyo. This "Low" had a gradient for Tokyo of 10 mm. in about 220 km. This is an excessively steep gradient. Elsewhere it has already been stated that a gradient of one-tenth inch for 150 miles is a pretty steep gradient; it will be seen, then, that the former is fully four times as great as the latter. The tilting diagram shows the movement of the "Low" very well, and from the constants of the pendulum, the angular motion or tilting, is found to be 3″·5, a very large quantity. As the "Low" was *east* of Tokyo, the natural inference would be that the pendulum swing to the west towards the "High," which depresses the surface of the earth, but the opposite was found to be the case. On this interesting and important point Omori says: "The explanation of this apparently anomalous phenomenon is probably to be found in the accumulation of sea waters under the low

pressure centre to a degree greater than the amount of the diminution of the atmospheric pressure, thereby creating an increase in the resultant pressure at sea bottom."

In interpreting the above phenomenon it seems necessary to consider the effect of the hydrographic features and coast-line, for statically considered, the difference of barometric pressure over the ocean raises the water under low pressure to a height equal to the difference between the "High" and the "Low," to produce hydrostatic equilibrium. However, when the "Low" is along or near the coast and the waters from the ocean flow from the "High" towards the "Low," then there may be and probably is a piling up of waters along the coast due to configuration, which would not be the case on the broad ocean.

The other case is the record obtained at Mito (Japan) during a storm 22–24 March, 1907, given in *Bulletin of the Imperial Earthquake Investigation Committee*, Vol. II., No. 1. The atmospheric conditions were similar but not identical. The gradient in this case was 10 mm. in about 400 km., and, furthermore, two "Lows" moved simultaneously northeasterly along the Japanese Islands, one on each side, thereby materially changing the direction of the gradient and of the direction of tilting as well as of its progressive movement. From the constants given, the instrument used seems to have been identical with the one used at Tokyo. The tilting recorded at Mito was 3″·7, a little higher than at Tokyo, although the atmospheric conditions were seemingly somewhat less favorable. An additional record of interest in this case is the mareogram for the time of the storm. From it, it is seen that the effect of the "Low" on the coast is to raise the water 75 cm. or two and a half feet "higher than the level according to the usual tide movement." The direction of the tilting with reference to the position of the "Low" was similar to the case of Tokyo; Omori saying:

"The passage of the centre of the cyclone producing, as in the case of the storm on Oct. 10–11, 1904, not an elevation but the depression of the ground. This is probably due to the fact that the deep barometric cyclone was accompanied, or rather

followed, by an increase of the height of sea water to an amount greater than the equivalent of the barometric fall."

We see, then, from these two cited cases that the secondary effect of difference of atmospheric pressure for coastal stations may completely mask, in fact reverse, the direct pressure effect upon the earth's surface produced by an atmospheric gradient. These cases seem to make it clear that the direct gravity effect produced by the cumulating waters is very much greater than that of the difference of atmospheric pressure, and is of opposite sign.

It is found that, broadly speaking, the microseisms are more numerous during the colder season than during the warmer one, and some have sought therein a relationship of cause and effect. In our climate here we have a large range of temperature, during the past year of 127° F. (96° and −31° F.) During February, when the thermometer reached its lowest and we had some continuously very cold weather, the seismograph showed no evidence thereof. The connection is assumed to be from the fact that the frozen ground on the one hand lends itself for the better transmission of pulsations, and the other that the act of freezing itself sets up stresses and consequent oscillations that manifest themselves as microseisms. From extreme cold it does not necessarily follow that the ground is frozen to any great depth, such as the case during the past winter. The reason that there was very little frost in the ground, was that an early and heavy snowfall together with its subsequent accumulation to many feet, covered the earth with a mantle that the cold could not penetrate

By far the large majority of microseisms show themselves by a serrated record, "sawtooth" type as I designate them; more rarely are those of the "spindle" type, where the oscillations or rather the amplitudes rise and fall, increase and decrease, with a certain cadence, as in the vibrations of a string between two fixed points. The interval between the maximum amplitudes is very variable, varying from one to several minutes. The rate of increase and decrease of the amplitudes is less than

that produced by the damping of the pendulum, so that we can scarcely attribute it to the latter on the supposition that the pendulum itself is set in motion and the oscillation dies down by damping, the former to be renewed by a fresh impulse. The latter, however, would preclude a gentle increase, but instead would show a more or less abrupt beginning, which is not the case. Intermittent rhythmic vibrations of the ground, synchronizing with the period of the pendulum, setting the pendulum in motion could produce the phenomenon. Other suggestions might be made, but none seems satisfactory to explain the more or less rhythmic fluctuations in amplitude.

The validity of a supposed relationship between different phenomena, as cause and effect, is readily tried by predicting the effect when given the cause. This has been done with reference to the existence of a "Low" in the Gulf and a "High" over the Atlantic coast to the south, or in general by taking the daily weather map with its isobars and from it predicting the resulting microseisms. The result has in so far been satisfactory that in the large majority of cases the microseisms have fairly well answered in presence and magnitude the prediction. There are, however, still important outstanding differences that require further explanation. Just why the "Low" about the Gulf should have such an influence in the production of microseisms is by no means apparent. The two main physical features are the shallow Gulf and the St. Lawrence valley in which lies the Great St. Lawrence and Champlain Fault, 700 miles long, already referred to. As secondary, is the general trend of the Atlantic coast, and possibly that too of the Alleghany mountains.

On infrequent occasions there is a "Low" over the Gulf, another "Low" over Arkansas, while one "High" rests north of Lake Superior and another over Bermuda. When those conditions obtain with steep gradients we are pretty sure to have marked microseisms. The line of the "Lows," then, lies in the St. Lawrence valley, while that of the "Highs" is at right angles to the former. In this case the maximum strain is along the valley of the St. Lawrence, along the Great Fault, so that

from *a priori* reasoning marked microseisms might be expected.

In concluding this preliminary investigation of the well-marked microseisms recorded here, we will repeat that the presence of a "Low" over the Gulf surrounded by steep or fairly steep gradients on a given morning is indicative of more or less well-marked microseisms following at Ottawa that day.

It has already been stated that the large majority of microseisms have a period of about 6^s with small fluctuations. Why the fluctuations is by no means apparent unless it be the varying depth of the earth's surface involved. Even this supposition is not quite satisfactory for all impulses, vibrations of whatsoever nature must pass through the pier on which the instrument rests before being recorded, and the pier itself must have its own inherent and constant period.

On occasions the above common period changes to one of about one-half, or about 3^s, showing, however, a transition time during which there is an irregularity and interference, so that their period is unrecognizable. At present no explanation can be offered for this sudden change. When the period is so short, the amplitudes are very minute, although visible to the naked eye.

From the tabulation it is found that the strongest and most numerous microseisms were recorded during the month of October, and the fewest and weakest during the summer months of July and August when the atmospheric barometric gradients were very long. Coming now to the next table in which the days for all the well-marked microseisms and for all the strong winds forecasts are given, we find that of these 100 days between the 1st July, 1907, and the 31st March, 1908, there were only 18 coincidences of such microseisms and such winds. It must be remembered, as already noted, that the winds are those of the forecasts and not recorded ones, as at present the Observatory is not provided with the necessary meteorological instruments. However, the probability of the forecasts is and has been high for years.

On these data and conditions we are led to infer that strong winds have little effect in causing microseisms by setting up pulsations over large areas of the earth's surface or crust, *i.e.*, the dynamical effect by friction or impact is not the governing factor in the production of microseisms. We are dealing here with the larger effect of strong winds upon large areas and not the local effect upon buildings, which, as is well known, are set in oscillation, and these in turn are communicated to the ground. When the building within which the seismograph is housed is large, the oscillations of the former will be recorded.

We have in the period July 2–March 26, one hundred days during which 51 marked or well-marked microseisms are recorded, and 36 strong winds are predicted. Considering the two phenomena as independent events, we see that the probability of the simultaneous occurrence of the two events is as great as the actual happening, *i.e.*, as far as the observations go there is very little to show any causal relationship between the two.

HIGH WATER IN LAKE ONTARIO

By Andrew Elvins

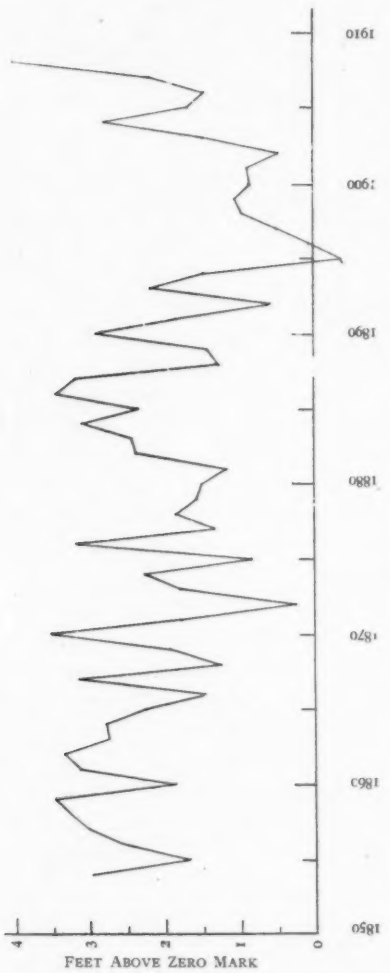
THE water of the Lake at Toronto has been very high this year. The Island Park has been submerged in many parts, and is unsuitable for picnic parties and atheletic games. The record kept at the Queen's Wharf since 1854 shows that June is the month in which the water reaches its highest point, and that June of this year, its height is above any previous year.

This is undoubtedly caused by precipitation in the lake region. Twice the water was very low in June, near the zero mark, (in 1872 and 1895), and it kept low until 1902. Since that time it has been fluctuating, but on the whole rising, until it reached 4 feet this June, the *highest point ever attained*.

The clearing of our forests will cause the accumulated snow of the winter to thaw rapidly in the spring, and this may be the cause of the periodic variations ; but the fact that the level fell from 3 ft. 6 in. in 1886 to zero in 1896, and has been rising ever since until it has reached 4 ft. in 1908, would seem to show that we may look to cosmic conditions,—possibly to meteoric matter entering the earth's atmosphere,—for an explanation.

Our forests have been cut down and reduced *constantly* and *regularly*, and are less in extent now than formerly, while our Lake is higher than ever before.

The accompanying diagram shows the levels for June for the last 55 years.



Mean Level of Water at Toronto for June from 1854 to 1908. The Height is above the Zero of the Harbor Commissioners' Gauge at the Queen's Wharf; this Zero is 244·79 Feet above Mean Tide at New York.

MEETINGS OF THE SOCIETY

AT TORONTO

June 16.—Mr. J. S. Plaskett, B.A., of the Dominion Observatory, Ottawa, read a paper on "The Optics of the Telescope," profusely illustrated with lantern slides and experimental demonstrations.

At this meeting the new constitution of the Society as drawn up and approved by Council was read and accepted by the Society, to come into effect on December 1st next. Copies of the constitution will be distributed to the members in due course.

June 24.—An open air meeting was held on the University Campus and a number of large telescopes were placed at the disposal of the public. Several hundreds of visitors availed themselves of the opportunity of viewing various celestial objects. Among the instruments on the lawn were the Society's 4-inch Cooke, Mr. A. R. Hassard's 4-inch, and Hon. Joseph Pope's 4-inch Grubb.

AT PETERBOROUGH

June 4.—There was a large attendance at this meeting, which was held at the home of Mr. H. B. Collier. In the afternoon two telescopes were placed on the lawn at the disposal of the members and their friends, and the President Rev. Dr. Marsh and Dr. Milton Stratton of Napanee answered the various questions which presented themselves to the observers as they viewed the Sun through the instruments. The sky was unclouded and the large sun-spots were studied with advantage.

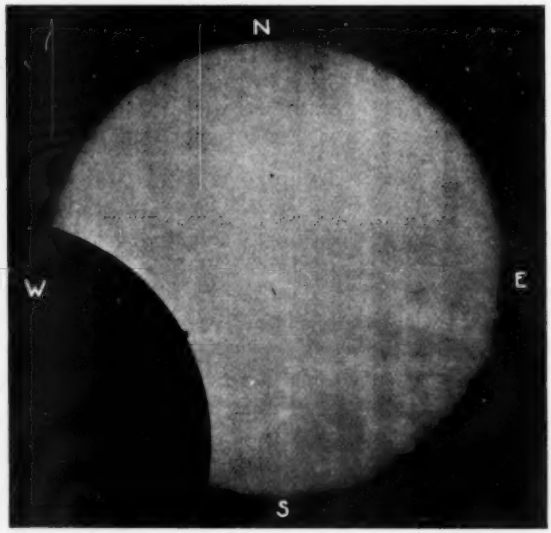
In the early evening a large number of members again assembled and viewed the Moon, Venus and Jupiter.

About 8.30 Mr. and Mrs. Collier opened their home to the Society, and the president introduced Dr. Stratton, the speaker of the evening, who spoke on "The Medium of Vision." The lecturer explained the structure of the eye, and the effects of light on the eye, how a sensation is changed into a perception. He also showed why it is impossible for a normal eye to have more than one perception of the same object at the same time. The subject was treated in an extremely lucid and interesting

manner, and hearty thanks were tendered to Dr. Stratton. Thanks were also presented to Mr. and Mrs. Collier for their hospitality. E. F.

The Partial Solar Eclipse of June 28, 1908

Whilst partial eclipses of the Sun are not of great scientific value, they are nevertheless interesting and not without profit. They at least stimulate the amateur observer, and afford an opportunity for practice to the astronomical photographer.



The partial eclipse of the Sun of June 28th of this year as observed in Springville (near Peterboro') was indeed a very pretty spectacle, and as it occurred at the time of Sunday school and church service, hundreds of worshippers saw it through a 5-inch telescope and other instruments on the manse lawn.

Six plates, ($8\frac{1}{2} \times 6\frac{1}{2}$), were exposed, five of which made good negatives. The apparatus used was a 5-inch Brashear refracting telescope, 75-inch focus, with a Beck telephoto amplifier placed inside the focal plane of the objective. With a camera extension the solar image was projected on the sensitive plate. The objective was stopped down to $1\frac{1}{4}$ inches aperture, and the solar image of the objective enlarged by the Beck element from $\frac{3}{4}$ of an inch to $5\ ^{9}/_{16}$ inches in diameter. An aluminum drop-shutter, with a slit $^{1}/_{16}$ in. wide, was placed immediately behind the amplifier and was driven as rapidly as possible through the cone of light by rubber bands, making an exposure of about $\frac{1}{1000}$ of a second. The plates used were Ilford chromatic. The writer finds these plates of fine grain, free from flaws and very uniform.

The first plate was exposed a short time before the eclipse came on, and shows four disturbed areas on the solar disc. One on the eastern limb and one on the western limb not well defined, one very small spot nearly in the centre of the Sun close to its equator, and a larger spot in the south-western quadrant. The fourth photograph of the series is here reproduced and shows three of the above-mentioned disturbances with the lunar limb cutting the larger spot. While the writer took the church services his son James was left in charge, and the accompanying photograph was taken by him.

Two objects were in view in photographing the eclipse :—

1. To expose a plate when the lunar limb would cut a sun-spot, that a comparison might be made of the darkness of the Moon and the sun-spot. On the negatives the dark side of the Moon appears distinctly darker than the sun-spot, but the difference is not so noticeable on the solio print or the reproduction here given.

2. If there is an atmosphere on the Moon—as some astronomers think—even though it be very attenuated, it might perhaps be noticeable to some extent by a faint penumbreal veiling forward of the lunar limb. No such veiling is visible on the negatives, the lunar limb being sharply cut.

D. B. MARSH.

NOTES FROM THE DOMINION OBSERVATORY

Astrophysics.—The cœlostat telescope for use in research on the Sun was installed over a month ago, and notwithstanding some apparently unfavorable conditions gives excellent solar definition.

It is placed due north of the west wing of the Observatory in a fully louvred building in which the air has consequently free circulation. The central part, which covers the cœlostat and secondary mirror, is larger than the others, and rolls back on wheels over the rear part containing the concave mirror, in order to uncover the cœlostat to the Sun. The course of the beam of sun-light is from the cœlostat south to the secondary, then north to the concave mirror, and south under the secondary mirror through the louvred passage and a short tunnel into the basement of the Observatory. The cœlostat and secondary mirror are each 20 inches in diameter, and the concave is of 18 inches diameter and 80 feet focus, forming an image of the Sun about 9 inches in diameter.

There is installed in the optical axis of the concave mirror a Littrow form of grating spectroscope containing a combined collimator and camera lens of 6 inches aperture and 23 feet focus and a 6-inch plane grating by Michelson. This spectroscope is arranged to rotate around the axis of collimation in order to place the slit in any desired position angle, (for example, parallel to the Sun's equator), the angles being read by a circle graduated to degrees. The camera, which is about 4 inches below the slit, takes plates 12×2 in size, is movable up and down by rack and pinion to take a number of spectra side by side, and can be tilted if required to obtain the whole length in sharp focus.

This spectroscope will be used in investigations of sun-spot spectra and in determinations of the solar rotation period, besides toher spectroscopic work.

A camera for making direct photographs of the 9-inch solar image formed by the concave mirror will shortly be erected, and investigations of the intensity of the solar radiation will be undertaken as soon as possible.

Mr. J. B. Cannon, B.A., has recently been appointed assistant in astrophysics, and is engaged in measuring star spectra for the determination of radial velocities.

J. S. P.

Meridian Work.—The reconstruction of the piers in the Transit Room and Meridian Circle Room is now in progress. The demolishing of the old piers is completed, and the work of excavation and rebuilding is proceeding as fast as circumstances will permit. During the excavation it was found that the soil was completely saturated with water, which goes far toward explaining the failure of the old piers. They are now being sunk several feet deeper into the earth, and a carefully planned system of drainage will, it is hoped, eliminate any further trouble. As the depth of the piers prevented efficient drainage into the regular sewer system, a special cistern is being built, which can be pumped out from time to time. The collimator piers are provided with pits to allow access to the underground marks.

When the graduated circles were received from the makers, after having been re-turned and re-graduated, the instrument was mounted on temporary piers in a small outbuilding for testing purposes. While the circles are now very much improved, there appear still to be some slight irregularities (though not very serious ones) in the shoulders on the axis against which they bear ; it is hoped, however, that these can be removed by a judicious process of polishing and scraping. It developed also that in the process of re-turning the circles the fact had been lost sight of that the distance from the plane of the graduations to the end of the axis must be exactly the same for both circles, else the microscopes would not be in focus after reversal. As it now stands these distances differ by about a millimetre, a defect which will require to be remedied.

There are also further slight modifications required in several of the parts, so that it will be some time, even after the completion of the piers and of the building, before the instrument can be got into regular service.

R. M. S.

The Seismograph—Bosch Photographic—Magnification 120.—June is a month fairly quiescent for microseisms, and so it was again this year. Although some very minute ones were recorded, the maximum amplitude recorded was on June 24, which, however, was only 0·3 mm. The quiescence continued almost throughout the whole of July. On July 26 were recorded the first typical "sawtooth" tremors since May; they continued for about two days and reached a maximum (double) amplitude of 1 mm. It may be interesting to note that last year after a similar period of quiescence the first well-marked microseisms occurred on July 29, three days later than this year.

On June 16 a local shock was felt by many people in the City; it was reported, too, from Rockland, Cornwall and Ogdensburg, the latter about 60 miles southeast of Ottawa. The shock was manifested by rattling of windows and a swaying motion. The writer did not feel it. Personal inquiries showed that the physiological effect was of the most divergent nature. It would appear that, coming suddenly, as it always does, before people come to their senses the thing is over, and nothing but the simple event is left clearly in their minds. As was to be expected, the seismograph which is especially adapted by its period for distant earthquakes, showed little disturbance. The shock occurred at $3^h\ 41^m\ 52^s$ P.M. (Eastern Standard Time) and 16 seconds later the pendulum was pursuing its even tenor. When our limestone formation settles the fractional part of an inch, it is enough to alarm the people.

On June 18 a distant quake was recorded, of which the following are the data:—

	N–S Component	E–W Component
	h m s	h m s
1st P. T. began	10 46 38 G.M.T.	10 46 44 G.M.T.
2nd " " "	10 52 16	10 52 20
P. P.	11 01 40 ?	11 01 28 ?
Duration	1	1 15
Max. Double amplitude	1·6 mm.	1·8 mm.

From the interval between the 1st and 2nd P. T. the distance to the epicentre is estimated at 3,800 km.

Shortly after noon on June 30 a small quake was registered which lasted about 30 minutes. The only phase that could be identified was the arrival of the first preliminary tremors (1st P. T.) at $17^h\ 51^m\ 28^s$ G.M.T.

Immediately before 8 A.M. on July 8 another distant quake was recorded, as follows :—

	N–S Component	S–W Component
	h m s	h m s
1st P. T. began	12 58 16 G.M.T.	12 58 37 ? G.M.T.
2nd " " "	13 04 51	13 04 52
P. P. "	13 16 ?	?
Duration	1 15	1 10
Max. double amplitude	1 mm. at $13^h\ 20^m$?	3 mm. at $13^h\ 20^m$

The epicentre is estimated at about 5,000 km.

On July 16 at noon a distant earthquake was recorded ; only the first and second preliminary tremors are recognizable of the various phases, and from them the distance is estimated at 6,900 km. The following are the data :—

	N–S Component	E–W Component
	h m s	h m s
1st P. T. began	17 00 30 G.M.T.	17 00 32 G.M.T.
2nd " " "	17 08 52	17 08 51
P. P. "	. . . . .	. . . . .
Then almost total quiescence,		
A fresh impulse	17 49 40	17 50 10
Duration	1	1
Max. double amplitude	2·5 mm. at $17^h\ 54^m$	. . . . .
End sudden	17 59 28	17 59 40 fairly sudden.

From subsequent Press despatches the above appears to be the record of the Tacna and Arica, Chile, earthquake, which broke the cable between Arica and Lima. From the recorded amplitude it would appear that the hypocentre was at no great depth.

In connection with the study of microseisms, of which less is known than of macroseisms, there have very recently been installed at the Observatory a Shaw–Dines microbarograph and a Richard statoscope. The former is "an apparatus designed to magnify and record the minor and sudden fluctuations of the atmosphere as opposed to the general barometric surges. It records the small variations on a scale magnified 20-fold, the general surges being practically obliterated through the operation of a small leak. It thus records comparatively rapid variations, and no others. . . . . The instrument consists of a small, closed vessel containing air, which communicates with a mahogany box containing mercury, on the surface of which floats, mouth downwards, a light hollow cylindrical bell. The air is inclosed in a large japanned metal chamber, the space between the two being filled with a non-conducting material. The movements of the bell are transmitted to the chart by means of a delicate system of leverage. The chart is wound round a drum actuated by clockwork, and making one revolution in 24 hours."

The statoscope, a French instrument, is also a microbarograph with 25 magnification. It is composed essentially of a series of sensitive membranes (like an aneroid) within a reservoir, which is closed by a tap, and is surrounded by insulating material. The movement of the membranes is communicated by a system of levers to the style which records on a drum revolving once in 52 minutes.

It may be noted that a difference of elevation of one metre is recorded on this instrument by a deflection of two millimetres.

Both instruments are working well, but as they have been only installed for a week or so, it is too early to discuss the relationship between the records of these sensitive instruments and that of the seismograph.

At this writing an Open-Wound Thermometer, range – 40° C. to 40° C., with Callendar Electric Recorder is being installed. The records of this instrument, too, are to be used in the study of the behavior of the seismograph and seismology in general.

O. K.

ASTRONOMICAL NOTES

HALLEY'S COMET.—Prof. O. C. Wendell, of the Harvard College Observatory, says: "In the *Monthly Notices of the R. A. S.* for March, 1908, Messrs. Cowell and Crommelin have given provisional elements of a new orbit of Halley's comet for its return to perihelion in 1910. In the calculation of these elements, nearly all the perturbations have been included except those of the plane of the orbit and which affect the node and inclination. In the case of these last two elements, the corresponding elements of Pontécoulant's final orbit have been provisionally adopted, pending the calculation of the perturbations of these elements. Any subsequent change, however, will be so small that an ephemeris for finding purposes has been calculated by them with the coöperation of Dr. Smart, beginning with October 1, 1908, and extending to July 13, 1910.

As it is possible, however, that the comet might be found before the first of October of the present year, I have calculated an ephemeris from the same elements for intervals of ten days, extending through August and September. As will be seen in the table below, the comet will be over two hours from the Sun on the first of August. This angular distance steadily increases so that by the last of the month, the comet will be about four hours from the Sun and by the last of September, about six hours.

Date	Halley's Comet						Sun			
	R. A.			Dec.		Millions of Miles from		R. A.		Dec.	
						Sun	Earth				
	h	m	s	°	′			h	m	°	′
1908 Aug. 2	6	34	21	+13	27·9	654	731	8	49	+17	49
" " 12	6	37	53	+13	20·5	647	715	9	27	+15	2
" " 22	6	41	0	+13	11·4	639	696	10	4	+11	50
" Sept. 1	6	43	37	+13	0·9	632	676	10	41	+ 8	20
" " 11	6	45	33	+12	49·2	625	654	11	17	+ 4	37
" " 21	6	46	41	+12	36·7	617	632	11	53	+ 0	46
" Oct. 1	6	46	54	+12	23·9	610	608	12	29	− 3	8

SUMMARY REPORT OF THE WEATHER IN CANADA JUNE, 1908.

TEMPERATURE.—The mean temperature for June differed little from normal in any part of Canada. From the Lower Mainland of British Columbia, however, to New Ontario, and also over the Northern portion of the Gulf of St. Lawrence, values were slightly subnormal, whilst elsewhere in Canada a mean temperature in excess of the average was recorded. In the Western Provinces negative departures from average amounted to from 1° to 2°, while in Ontario, Quebec and the Maritime Provinces positive departures were from 1° to 4°.

For the highest and the lowest temperatures at various stations see the accompanying table.

PRECIPITATION.—The chief feature of the distribution of precipitation during June was the excessive amount over the Central and Northern portions of Alberta and Saskatchewan, where the fall was almost generally more than twice the average amount, while with local exceptions, the amount of rain recorded elsewhere in Canada was much less than normal.

JULY, 1908.

TEMPERATURE.—The mean temperature of July was above normal throughout the greater portion of Canada. A normal or slightly subnormal value, however, was recorded in Eastern Saskatchewan, the greater portion of Manitoba and the Rainy River and Thunder Bay Districts of Ontario.

Over the Interior of British Columbia, Southern Alberta, Quebec and the Maritime Provinces positive departures of 4° were general.

For the highest and the lowest temperatures at the various stations see the accompanying table.

Temperatures for July and August

Station	July		August	
	Highest	Lowest	Highest	Lowest
Yukon				
Dawson	84	34	84	39
British Columbia				
Agassiz	91	41	92	42
Barkerville	70	32	82	36
Cowichan			90	42
Kamloops	92	42	102	42
New Westminster	86	44	97	45
Spence's Bridge			108	46
Vancouver	80	45		
Victoria	88	46	83	44
Western Provinces				
Almassippi			96	34
Battleford	90	42	94	44
Birtle	82	37		
Brandon			94	40
Calgary	82	36	89	40
Carman			92	36
Cypress River			93	40
Edmonton	79	34	88	42
Medicine Hat	95	40	94	44
Minnedosa	84	32	90	42
Moosejaw			101	41
Pierson			92	34
Pipestone	95	30	93	42
Portage la Prairie	90	34	95	44
Prince Albert	86	32	92	44
Qu'Appelle			90	36
Regina	86	30		
St. Albans			98	48
Swift Current	96	33	92	46
Virden			93	40
Winnipeg	88	32	97	37
Ontario				
Agincourt	89	42	92	49
Aurora			92	46
Bancroft	87	33		
Beatrice			88	42
Bloomfield	87	43		
Brantford			93	47
Bruce Mines	83	34	87	43
Clinton	89	33	90	43
Cockburn Island			87	43
East Toronto	85	41	91	50
Gravenhurst	91	35		
Haliburton			92	41
Huntsville	85	37	87	46
Kingston	82	42		
Kinmount			90	39
Lakefield	85	42	91	51
Lucknow	87	31	90	42
Meaford	90	42	90	50
Midland	90	50	95	55
North Gower			97	47
Otonabee				
Ottawa	92	41	94	50
Owen Sound	91	36	94	47
Paris	90	41	93	50
Parry Sound	90	36	94	48
Peterboro'	87	40	93	47
Point Clark			99	55
Port Arthur	83	33	90	42
Port Burwell			90	48
Port Dover	88	38	92	50
Port Stanley	85	38	91	46
Rockliffe	91	34	92	46
Ronville			89	43
Sarnia			96	55
Southampton	87	38	88	48
Stony Creek	94	41	96	52
Stratford			89	47
Toronto	88	43	92	54
Uxbridge	88	36	92	49
Wallaceburg			93	48
Welland	91	44	96	50
White River	84	26	88	32
Windsor				
Quebec				
Brome	86	31	92	40
Father Point	78	32	80	42
Montreal	87	44	92	54
Quebec	88	36	90	46
Sherbrooke				
Maritime Provinces				
Charlottetown	81	37	89	46
Chatham	94	33	96	45
Fredericton	92	37	90	45
Halifax	86	50	90	46
Hamilton, P.E.I.				
Moncton	89	34	90	30
Parrsboro'			86	40
Port Hastings				
St. John	78	35	84	50
St. Stephen				
Summerside			89	45
Sussex			90	40
Sydney	83	35	86	46
Yarmouth	70	40	80	44

PRECIPITATION.—The precipitation during July in Canada was very generally less than the average, and this was paricularly the case in the Western Provinces, and especially in Southern Saskatchewan, where the total amount was but 26 per cent. of the average. Marked deficiencies also occurred in New Ontario and Northern New Brunswick. Some few localities in Northern Manitoba, Central Ontario and Southern Nova Scotia reported a slight excess, generally due to thunderstorms of a very local character.

METEOROLOGICAL OFFICE,
TORONTO, CANADA,
August 28, 1908.